

## **Studies of Work: Achieving Hybrid Disciplines in IT Design and Management Studies**

**John Rooke & David Seymour**

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# **Studies of Work: Achieving Hybrid Disciplines in IT Design and Management Studies<sup>1</sup>**

## **Abstract**

We explore the relationship between ethnomethodology (EM), ethnography and the needs of managers and designers in industry, considering both ethnomethodological and industrial criteria of adequacy and explicating their relationship through the concept of “audience.” We examine a range of studies in this light, with a view to their possible candidacy as hybrid studies and identify three types of application of EM studies of work: market research, design, and business improvement. Application in the first of these fields we dub “anthropological,” in that it consists in studying and reporting back on the ways of exotic people (customers). This is the application most commonly found in studies of computer supported co-operative work (CSCW). A second CSCW application, “technomethodology,” involves the introduction of EM concepts into the design process. A further application, dubbed “holding-up-a-mirror,” involves reporting back to members of a setting upon their own activities. We argue that technomethodology and holding-up-a-mirror both offer the possibility of creating hybrid disciplines. We consider the objection that improvement and design involve the introduction of value judgements that threaten the practice of EM indifference, arguing that action research can serve as a guarantee of unique adequacy (UA) by testing the researcher’s understanding as analysis in action in the setting. Furthermore, the standard of reporting required by the UA criterion contributes to the effectiveness of proposed solutions.

**Key words:** Ethnomethodology; Studies of Work; Hybrid Disciplines; Unique Adequacy; Ethnomethodological Indifference; Technomethodology.

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### **Ethnography and Ethnomethodology (EM)**

Harold Garfinkel (1988/1991) offers an eight point definition of ethnomethodological (EM) studies and phenomena. This can be seen as identifying four aspects of EM:

1. that its phenomena are substantive, unique, local, reflexively produced and ubiquitous;
2. that they are available through EM policies, notably the policies of EM indifference and the Unique Adequacy (UA) requirement of methods;
3. that they are unavailable to the various conventional procedures of formal social analysis and, while necessary to the practice of such analysis, are properly absent from it;
4. that they “specify 'foundational' issues, in and as the work of a 'discipline' that is concerned with issues of produced order in and as practical action” (1988/1991: 16).

It is arguable that the use of EM policies alone is sufficient to distinguish EM phenomena. These phenomena are by definition available through EM policies and not through any policy of formal/constructive analysis. Further, we suggest that EM phenomena are the only phenomena available through EM policies. It is arguable that these phenomena are necessarily foundational. However, we will suggest that the foundational nature of EM phenomena is itself context dependent, an idea we will explicate through the concept of “audience.”

The policy of the UA requirement is sufficient to distinguish between EM and ethnography. Ethnography, or participant observation, is a research practice in which researchers enter settings and learn about them principally through instruction by other members of those settings. The weak UA requirement stipulates that to analyse a setting adequately, we must know what any member of that setting would ordinarily know about that setting. This can be regarded as a criterion for adequate ethnography: that the author of an ethnographic report can produce an adequate account only to the extent that s/he appreciates

the understanding of the research setting which any other member of that setting would have. However, there is a difficulty in applying this criterion in specialist settings: readers of a report cannot know for certain if that report meets the weak UA requirement, without having themselves attained vulgar competence in the setting. Thus, only specialists are ideally placed to make this judgement. For the most part, sociologists who are recipients of a report can only assess its credibility. Usually, these recipients must rely on such assessment methods as (for instance) the reliability of the author, the logical consistency of the account, and its consistency with other known phenomena.

The nature of EM phenomena is guaranteed by observing the strong requirement of UA. This criterion is possible because the methods that participants in a setting use to establish the ongoing order of that setting are sufficient to the purpose of producing an account of that setting. For EM, the use of any other methods must involve some distortion; to import methods from outside the setting would contravene the policy of EM Indifference. A consequence of the application of the strong requirement is that studies can be categorised according to whether or not the ethnographer has maintained EM indifference in the study. EM can, therefore, be a form of ethnography, but not all ethnography qualifies as EM.

Notwithstanding these concerns, Garfinkel continues to see the relationship with social science as indispensable: “[t]he achievements of the worldwide social science movement are the distinctive and fundamental topical interests of Ethnomethodological alternate studies of social order” (2001: 1). A further criterion by which EM studies should be evaluated then, is their viability as exemplars of an alternative to social science.

### **Topicalizing “Audience”**

Managers who employ researchers, or research findings, apply criteria of usefulness to the research. In this paper we investigate applications of EM studies of work that may be found useful by managers. These are initially categorised into two different approaches,

differentiated by the nature of the “audience” for the research. The concept of “audience” is used here to denote any member, or group of members who receive and evaluate the deliverables of the research: thus an alternative term might be “readership.” In common with other academic disciplines, sociology makes little attempt to topicalise the process of delivering its research to an audience. This process is one of the essential, but unnoticed features of the mundane work of research. As such, it is itself a natural subject for EM study.

The question of audience is touched upon by Harvey Sacks. In his discussion of the “commentary machine,” he observes that

the sociologist seeks to warrant his description of activities as scientific. He seeks to write a description which would be produced by a colleague observing another cycle, or which could be used by a colleague for analyzing the machine’s course of activities. (1963: 8)

Here, the audience is a community of sociologists which applies criteria of scientific practice to the study (notwithstanding that the precise nature and relevance of these criteria may be debatable). As EM originally developed, it can be understood as addressing either this sociological audience, or a specialist audience of ethnomethodologists that was emerging within the wider discipline.

It seems to us that the concept of “hybrid” studies brings this conventional relationship between ethnomethodology and its audience into question. The intention to bring the foundational insights of EM studies of work to various other disciplines would seem to imply an audience for these studies within those disciplines. However, it is not clear to what extent such an audience can be said to have emerged. There seems to be little evidence, for instance, that Eric Livingstone’s (1986) exploration of the ethnomethodological foundations of mathematics are of any great interest to mathematicians. More recently, studies of work have gained widespread acceptance in computer software design. However, practitioners such as Max Travers and Dave Calvey (2001) or Andy Crabtree (2002) express some unease

about the extent to which their non-EM audiences appreciate the import of such studies and also what the extent of their impact on the design process might be.

The difficulty involved in communicating the fundamental insights of EM to a professional or scientific audience has been addressed by Graham Button and Wes Sharrock, who observe that:

In order to engage in natural or social science it is conventionally argued that it is necessary to adopt the formal practices of the science and abandon the practices of common sense. Within both the natural sciences and the social sciences the reliance that is made upon the vulgar competencies of common sense thus go largely and systematically unrecognized. (1995: 253)

However, Button and Sharrock have discovered for ethnomethodology a domain of practice among computer scientists and software engineers in which attempts are made to “transform the mundane practices of programming into professional practices,” thus explicitly recognising “the role of mundane practices in the professional work of programming.” They argue that:

If computer science can recognize its reliance upon enabling vulgar competencies in some areas of its professional practice, it may, in principle, be amenable to having its reliance in other areas explicated, and in that explication it may find ethnomethodological studies of its work a resource for the development of its professional practices. (1995: 254)

However, EM studies of work carried out in the context of computer science and software engineering have not been primarily studies of designers and the design process, but studies of *other* workers, for the *benefit* of the designers. These studies focus on the description of mundane work practices in settings where the introduction of new technology is contemplated, or has taken place. Thus, hybrid studies may have at least two types of audience in addition to the conventional academic one. The first is members who are

interested in providing goods and services to members of the setting under study, or to members of similar settings. Studies addressing this type of audience may be thought of as adopting an anthropological approach, in that they report upon the activities of exotic people, in this case customers or potential customers for technological innovations. The second audience is members of the setting under study, themselves. This approach may be thought of as “holding up a mirror,” in that looking in a mirror allows self-observation in a way that reveals otherwise unnoticed features.

### **Market Research: An Anthropological Application**

The anthropological application involves reporting to managers or designers on the activities of unfamiliar populations. For market research, these populations are customers, or prospective customers. In terms of current “management-speak,” the intention is to “help designers to get closer to their customers.” Reports are read as straightforward ethnographies of workplaces and work activities. Such reports may be ethnomethodological, in the sense that they conform to the strong criterion of UA, or merely ethnographic, in that they conform only to the weak criterion. It is arguable that EM reports have two features that make them more suited to such an application than other ethnographies. First, the concern to focus on the haecceities of mundane practices facilitates the kind of detailed analysis that can inform effective design. The EM report supplies precisely the unnoticed but vital features that might be overlooked in less empirically grounded glosses. Second, the attempt to meet the strong criterion of UA ensures that the customer’s working methods are presented with minimal distortion due to the personal or theoretical leanings or speculations of the researcher. We will now briefly examine some studies of this type to highlight their audience orientation.

James Pycock and John Bowers’ (1996) report on designers in the fashion industry supplies details of fashion designers’ work for the benefit of designers of CSCW systems. They specify several ways in which such reports can inform the development of CSCW

systems. Thus, the intended audience for the paper would seem to consist of CSCW designers and ethnographers engaged in informing the design process. Although the authors do not refer to EM, or UA, it seems reasonable to assert that the paper does conform to the strong requirement of UA for two reasons. First, it contains a detailed report of the activities of the designers, which is clearly based on close observation. Second, both the reporting and discussion is free of speculative theorising etc. and is conducted in the fashion designers' own terms. On the other hand, there is no indication that the paper is directed towards, or would have any interest to, members of what Garfinkel calls the "worldwide social science movement" (2002: 91). More importantly, perhaps, it is not immediately apparent that the paper topicalises any achievement of social science. It is not apparent, then, that the study is in any way representative of an alternative to social science, unless an EM informed discipline of CSCW is itself seen in this light.

It is arguable that the paper does fulfil another important criterion of EM studies, in that it makes a foundational contribution to the discipline of design. Much here depends on what the term "foundational" is taken to signify. We might argue that in explicating the haecceities of work processes, the study makes a foundational contribution by supplying the missing "what more?" of design (Garfinkel 1996). This "what more?" is constituted in the lived details of doing fashion design. In this sense, "foundational" does not imply a generic feature of systems design, but rather a *necessary* feature. Thus, in order to do effective system design for an activity, it is necessary to found that design upon a specific knowledge of that activity.

Pycock and Bowers is typical of most of the anthropological studies, but two recent exceptions may be noted. Max Travers' (2001) study of work in a small office similarly offers descriptions of the everyday work practices of the settings he has researched. However, rather than addressing designers, or even ethnographers involved in the design process, he explicitly targets his remarks to a sociological audience. Again, the strong UA



requirement is met. Indeed, little distinguishes the style of reporting from Pycock & Bowers, other than the explicit intention of the paper, which is to inform “theoretical questions about technology, de-skilling, entrepreneurship, authority relations, the surveillance society, professionalism or social change” (2001: 83). In this way, the relationship to social science is explicitly maintained. However, it is difficult to see precisely how this relationship can be formulated in EM terms. It is clearly possible to argue that the study demonstrates a “distinctive and fundamental” interest in the achievements of social science. It might be that the relationship to sociology consists in the ethnographer supplying the haecceities of settings for the benefit of social theorists. However, this would constitute a very different relationship from that intended by Garfinkel, who has long insisted that EM studies are an *alternative* to social science studies and not supplementary to them. This intended relationship is more deeply confounded by Steve Mann, Jason Nolan & Barry Wellman (2003), whose unexplicated use of social theory and polemic undermine the EM indifference of their study (though see our comments on Goode below).

### **Towards Technomethodology**

Bowers, Button & Sharrock’s (1995) study of work flow technology in a print works also seeks to make a contribution to design practice, in that it offers suggestions for improvements to the design process. The study describes the work activities of the printshop, as they relate to the technology, explicating the tasks and problems faced by members and the ways they are dealt with. In this case, difficulties arise from the technology itself and these are described along with the methods employed to overcome them. The paper concludes that designers should give more attention to the way technology organises workflow and how this organisation may conflict with other organisational imperatives. Here the audience is once again designers who are addressed with a report that meets the strong requirement of UA. However, the emphasis is not so much informing the design process as critiquing it. While

still acting as anthropologists, explicating the ways of print workers for the benefit of designers, the focus is equally upon the consequences of the design process itself. The authors can be said to be “holding up a mirror” to the design process.

However, Button & Paul Dourish (1996) suggest that the development of a truly hybrid study requires a shift from critique of the design process to becoming part of design practice. This application involves more than the mere anthropological reporting suggested above, as is made clear in their practical application of the technique (Dourish & Button 1998). Rather than simply reporting on work processes, they are involved in supplying EM-derived concepts to the design process. In doing so, they seek to create a discipline of technomethodology, a CSCW design practice that does not merely learn from ethnomethodologists, or from EM accounts, but learns from EM itself. In making the shift, they establish a particularly strong claim to the creation of a hybrid study. They are able to do so because EM refuses theoretical status to a distinction between “technical” and “social” considerations<sup>2</sup>. Thus, EM ceases to be supplementary to the design process and instead becomes integral to it, constituted as a discipline that can claim ownership of design concepts.

In Button & Dourish’s conception, each application of EM represents a step on the road to hybrid EM-design. In making this journey, EM resolves a crucial paradox in CSCW:

that the introduction of technology designed to support ‘large scale’ activities while fundamentally transforming the ‘small-scale’ detail of action can systematically undermine exactly the detailed features of working practice *through which* the ‘large scale’ activity is, in fact, accomplished. (1996: 4)

However, in doing so, it generates a second paradox for EM itself, the “paradox of technomethodology”:

[g]iven the concern with the particular, with detail, and with the moment-by-moment organisation of action. . .the face of the unavoidably transformational nature of the

technology and system design in working settings, it would seem that ethnomethodology becomes relatively powerless. Its tradition is in analysing practice, rather than ‘inventing the future.’ (1996: 4)

They see the shift from critique to participation as one which provides a basis for the resolution of this second paradox, which they conceive to be the result of confusion over the differing relationships between generalities and particulars in the two disciplines. They point out that although EM directs attention to the particular, while system design deals in generic abstractions, EM does generate generalities. It is the difference in nature between EM abstractions and those of system design that needs to be grasped: while the former are purely analytic, the latter are also generative. Crucially, EM abstractions are “context free, yet context sensitive”; their contribution to the design process consisting not of sets of rules that can be simply taken over and converted to generic tools for system design, but of sensitising concepts that stimulate particular sensibilities in the designer.

Crabtree (2002) develops these ideas further, treating the application of novel technologies as breaching experiments, thus explicating the schemes of interpretation which members adopt in using these technologies. He suggests a model of technomethodology which uses the consequences of these experiments to identify perspicuous settings which in turn provide guiding concepts for design.

### **Management Studies: Holding-up-a-Mirror**

Our own concern is with the development of hybrid management studies. Beginning with Egon Bittner’s (1965) incisive analysis of the concept of a formal organisational scheme, there is a growing body of EM work that is directly concerned with topics of interest to management studies. Contributors include Don Zimmerman (1971), Dave Francis (1984), Lucy Suchman (1987), Robert Anderson, John Hughes, & Sharrock, (1989), Tim Dant & Francis (1998), I. Koskinen (2000), Button & Sharrock (2002), Hughes, Mark Rouncefield &

Peter Tolmie (2002). However, the conscious creation of a hybrid discipline of EM management studies has as yet been given little attention.

In attempting to develop such a discipline, it is an important initial concern that, whereas design is concerned to model the activities of an end user, management studies are principally concerned to model organisational activities for the edification of the organisation's members themselves. For this reason, we see the holding-up-a-mirror approach as the more viable way of applying ethnography within this field. More precisely, we seek to apply it within the context of production management (see for instance Geary Rummler & Alan Brache 1995, Lauri Koskela 2000). The intention is to help members to reflect on their own practices as they seek to identify opportunities for improvement. The arguments given above, regarding the utility and status of anthropological reports, are equally applicable here. However, "holding up a mirror" implies the possibility of a different relationship between the researcher and other members of the setting under study. In this application, the members of the setting can become the audience for the research.

As noted above, this approach is much less developed than the anthropological one. However, David Seymour, Mazin Shammash-Toma and Leslie Clark (1997) provide a study that we use here as a candidate example of the holding-up-a-mirror application. Once again, the refusal to grant privileged status to a distinction between technical and social issues is a central characteristic of the research report. Features normally categorised in this manner are treated, as they appear in the setting, as belonging to a single process of activity. This enables the process to be treated holistically, rather than as a one-sided "engineering" or "social" phenomenon.

The research was conducted in collaboration with structural engineers and focuses on the achievement of specified depths of cover for steel reinforcement in concrete structures. Adequate cover is vital for the long term viability of such structures; if insufficient, it allows the penetration of water and salts, leading to corrosion. The research was carried out across

twenty-five construction sites, where the process of constructing a sample of walls and columns was observed and the cover achieved was measured to establish the extent of compliance with design specifications. Participants in the construction process, including designers, site engineers, managers, foremen and operatives, were interviewed both during and after construction.

Statistical analysis of the cover measurements revealed a high incidence of variation from specified values. This finding was made subject to two alternative explanations, deriving from two distinct construction management analyses, in conjunction with the interview data. The first of these, referred to as “the standard explanation,” distinguishes between design defects originating in the design office and construction defects originating on the construction site. The identifying characteristic of design defects is that the resulting design is physically impossible to execute. “Construction defects” is thus a residual category. This analysis is congruent with contractual arrangements in the construction industry that traditionally allocate responsibility and risk between the employer (responsible for the design) and the construction contractor (responsible for the execution). It suggests that 30% of defects originated in design, posing the question: were these a result of individual error, or of a failure of the systemic conventions of design production? The remaining 70% of defects are explained in terms of “inefficiency, poor workmanship, poor supervision, inadequate controls and so on” (1997: 9).

The alternative explanation takes up the question posed by the first, as to the respective roles of individual error and systemic failure. It also calls into question the attribution of 70% of defects to origins on site. It is based in the work of the International Group for Lean Construction, a loose network of academics, consultants and practitioners, who have been engaged for some years in the adaptation of developments in production management for application in the construction industry. A key distinction, elucidated by Koskela (1992) is between “transformation” activities that directly add value to the product and “flow”

activities, which include the resourcing of the work. This leads Glenn Ballard and Greg Howell (1998) to a conception of the construction process that places the work team at the centre of the analysis and focuses on the problem of making sure that the team is adequately resourced in order that it can function at full capacity. In this conception, design specifications can be seen as a resource to the construction process. This analysis is non-congruent with traditional contractual arrangements and highlights a conflict between contractual provisions and the optimisation of production, such that the contractor is forced to adopt an ineffective mode of quality control (Shammas-Toma, Seymour & Clark 1996). It brings into focus the range of variations in conditions that occur in the construction process that are beyond the predictive capacity of the designer. Finally, it proposes an alternative means of control that is vested in the “last planner,” defined as the planner whose “planning process is not a directive for a lower level planning process, but results in production” (Ballard & Howell 1994: 5). Seymour *et al.* implicitly endorse the Lean Construction analysis, advocating greater scope for professional and craft discretion and suggesting some guidelines for better design and organisation. Crucially, this account treats the design specification as a *resource* for the construction process, rather than a contractual obligation that *controls* the process (1997).

Some features that become relevant when this study is considered as a hybrid study are as follows. First, it is difficult to disentangle the EM input to the study from the Lean Construction input. To some extent, this is due to congruence between the two disciplines. A slogan of Lean Construction is that “construction completes design.” In focusing on the construction process and treating design as a resource flow into that process, Seymour *et al.*’s analysis can be seen as a realisation of that slogan. Simultaneously, we suggest, in supplying the haecceities of design specification and execution, the analysis is specifying foundational phenomena in and as the work of structural engineering design (see Garfinkel 1991, 2002). Second, while the study is partial in that it is committed to a particular analysis, in preference

to an alternative analysis found in the setting, it remains compliant with the strong requirement of unique adequacy, in that both analyses originate in the same construction industry setting that is their focus. Third, these analyses, along with other methods employed by members of the industry to sustain the organisation of the setting, thus executing the work in hand, are the sole interest of the study. This focus on members' methods constitutes a further basis for the claim as to the ethnomethodological nature of the study. Fourth, however, the study is claimed to be and undoubtedly is, a study in Lean Construction.

### **Indifference, Irony and Critique**

It might be argued of both technomethodology and holding-up-a-mirror that they depart from the policy of EM indifference. Is the intention to participate in design or improvement inimical to this policy? There is no doubt that commitment to a particular course of action within the setting may prompt a researcher to give an account that is competitive with others originating in the setting. On the other hand, engaging in participant observation, let alone the hybridisation of disciplines, must involve the researcher in precisely this kind of commitment at some point. As a basis for investigating this question further, we will revisit a previous debate between David Goode (1994, 1997) and Michael Lynch (1997) on a similar problem.

Goode (1994) characterises his research as ethnomethodological action research and argues that the policy of indifference does not entail a refusal to make value judgements within the setting. Thus, he defends his right to make a moral critique of practices he has observed: "Although these observations have a clear moral basis and are not indifferent in the sense that they are uncaring or neutral, I do not understand them as contradicting the policy of ethnomethodological indifference" (1994: 122).

Though it is sometimes suggested that EM indifference does require moral indifference to the topic, there is no support for this view in Garfinkel's own work, which refers to "an

indifference to the policies and methods of formal analysis” in social science (2002: 170). As such, it is a vital means of achieving the strong form of UA. The proper issue to consider here is whether the value judgement originates from within the setting under study, or is predicated on the application of *a priori* social theorising. Thus, Goode’s study qualifies as indifferent to the extent that his ethical critique is based firmly on the experiences and relationships of himself and others in the setting.

He also defends his point that “professional research about children with deaf-blindness essentially and irredeemably ‘misses’ the world of such children” (1994: 122). We see nothing exceptionable in this claim; it is entirely compatible with Garfinkel’s claim for EM *vis a vis* the social science movement (1988/1991, 1996, 2001, 2002). However, we would argue that Goode’s defence of it confuses the issue. He asserts that the principle of EM indifference “does not mean that one cannot find ironies or make statements about the relative efficacy of different sense-making procedures” (1994: 122).

Here, two problems arise. First, Garfinkel warns clearly and in the strongest terms against the use of irony: ironic studies are “useless” (1967/1984: viii). Irony is entirely destructive of the unique adequacy of an account, undermining any attempt at EM indifference by smuggling disguised value judgements into the report. It is a device which allows the analyst to give two incompatible accounts simultaneously.

Second, sense making procedures have their own criteria of efficacy by which they can be judged. It is not for the ethnomethodologist *as ethnomethodologist* to introduce additional criteria. We would suggest, however, that Goode is not acting as an ethnomethodologist when he makes such statements, but as a child care professional. As such, his remarks are not addressed directly to an EM audience, but to a professional one. As a professional researcher Goode is entitled to engage in such a critique of his colleagues, but to characterise this critique as ethnomethodological makes little sense.



Crucially, the prime focus of Goode's study is the understandings of the children themselves, as revealed in their interactions with their carergivers, rather than the sense making methods of other professionals. It is the intense observation of these interactions that validates his discovery of a world overlooked by professional research. His interactions with other professionals are reported in a different manner. His concern is not to show us how these professionals make the world sensible in the way that they do, but to show that they are wrong. The correct way to do this is as an ordinary member of the setting: it requires no recourse to EM policies, though it draws on EM discoveries.

Lynch's (1997) criticism of the study tacitly recognises these points by focusing on Goode's claims about the children's understandings. He argues that in using terms such as "skills," "purposes," and "alternative object reading," Goode is attributing qualities to the behaviour of the deaf blind children that it does not in fact possess. In his response, Goode (1997) concedes that some of his terminology may be inappropriate, but Lynch's criticism seems to be on a deeper level than this. It is founded on Wittgenstein's observation that rationality is socially sanctioned. He argues that:

an idiosyncratic departure from a conventional practice is not an alternative practice ... the book's account of 'understanding without words' is attenuated by Goode's sometimes strained attempts to turn Bianca and Chris into rational actors in private worlds (the inverse of the cultural dope). (1997: 375-6)

However, our own reading of Goode's project is that it is in fact a *plea* that the children's activities be granted such social sanction. Seen in this light, Lynch might be read as ruling out, *a priori*, the possibility that this sanction could be granted. This ruling would be based on a belief that their lack of spoken language condemned them to a private world. But it seems to us that this is just what is at issue. Goode's contention is that the children's world is not as private as many believe. This can be clearly seen in the structure of Lynch's own critique, when he observes that "Goode endows Christina with essentially private capacities

and competencies” (1997: 375). However, since we can only discuss Christina's “private capacities” because Goode has told us about them, we would argue that they must have a social nature. It would seem to us that it is Goode's contention that the rational nature of these activities is available to a careful and patient participant observer. Note, in this context, Joe Smith's observation on his understanding of a new behaviour of Bianca's: “I don't know now, but we'll figure it out if she keeps it up” (1994: 61). Thus, the pertinent question to ask of Goode's work is not, as might be taken to be implied by Lynch: is the rationality he seeks possible? Rather, it is: has he established its existence? In other words, Goode's claims should be judged, not on their philosophical basis, but on their detailed substantiation in his reported observations. It is Goode's ability to *make his interactions with the children work* which validates his claim concerning the rational nature of their actions and the unique adequacy of his study. Inasmuch as he succeeds in this, he is demonstrating the viability of his analysis in action and vindicating its claim to unique adequacy. To compare this analysis with that of other professionals in the setting seems to us to be beyond the remit or interest of EM.

The confusion stems from Goode's intention to address two distinct audiences and his failure to maintain an explicit analytic separation between them in the study. Each audience has its own criteria of adequacy for the accounts with which it is presented. These criteria are independent and sometimes conflicting. Thus, as an action researcher, Goode is required to maintain a critical relationship with conventional practice; it is expected that his research should make a contribution to the development of such practice. As an ethnomethodologist, he is concerned only with how (and not if) particular sense making procedures work; he is constrained from importing any novel analytic resource into the setting.

However, the crux of our argument is that the two approaches can be pursued simultaneously while locked in a relationship of creative tension. Action research adopts an experimental approach that requires the researcher to contribute hypothetical formulations to

the setting. Thus, it would seem almost mandatory for Goode to attempt ever more ambitious interpretations of the children's behaviour, to see if they can be vindicated. However, for action research to be successful, its prescriptions must be developed and implemented within the local setting. It is only by developing interpretations of the children's activities that are viable in the setting that valid action research findings can be achieved. This viability, we would argue, both guarantees the unique adequacy of the account and is simultaneously enhanced by it. Goode concedes that "changing society through studying it can make observing *analytic* EI [ethnomethodological indifference] even more difficult" (1997: 386). For those attempting to pursue action research and EM simultaneously, a temptation will always be present: to produce accounts for their rhetorical effect, rather than their unique adequacy. We suggest, however, that the pursuit of EM policies itself provides the researcher with a rhetorical resource. We also suggest that such an approach will prove a vital element in the development of successful hybrid studies. There are two important reasons for this. First, for a study to be truly hybrid in nature, it should meet criteria from not one, but both of the disciplines involved in its creation. Second, the action research approach has an inbuilt method of testing researchers' competence in the setting and thus their achievement of the weak UA requirement. In attempting to make changes in the setting, the researcher is involved in activities of interpretation, policy formation, and prediction, the success of which must be largely dependent upon competence in that setting. By making members of the setting under study into an audience for the research, the holding-up-a-mirror application provides a direct route for testing conformance to the weak UA requirement.

## **Conclusions**

We claim that most of the studies considered in this paper meet the strong requirement of UA. They nevertheless differ in important respects with regard to the possibility of their being regarded as hybrid studies. We have proposed that a way of explicating these

differences is to consider their relationships to the different audiences for which they were written. This concern with audience is apparent in Sacks' earliest work, where the problem of description is first raised (1963).

We have expressed a doubt that some of the most penetrating studies of work are not properly hybrid, as they make no recognised contribution to the disciplines they address. We have identified three types of study of work that might be said to make such a contribution and considered them in turn. Anthropological applications in design are addressed to informing the design process by supplying haecceities of customers' work. They contribute, to the design process, vital insights into the practices of the members of the setting of the study. However, it is difficult to argue that such studies make a *foundational* contribution to the discipline of software design itself. Here, the pursuit of EM policies enhances the value of ethnographic studies to the design discipline, but remains incidental to it. In addition, most of these studies do not explicitly address the achievements of social science. Travers is an exception to this last point: he addresses, in addition to designers, an explicitly sociological audience (2001). Here, the nature of EM as a sociological practice is clearly demonstrated. However, the relationship to mainstream social science remains opaque; he takes us no closer to the creation of a hybrid study.

In contrast, technomethodology presents a strong claim to be considered a hybrid discipline. Button and Dourish have traced the steps in its creation, showing how they generate and resolve paradoxes (Button & Dourish 1996, Dourish & Button 1998). Ultimately, their recommendation that ethnomethodologists should contribute sensibilities rather than rules is intellectually pleasing and probably correct. Crabtree offers what appears to be a viable model for practice (2002). Crucially, the prominence of EM concepts raises the EM contribution from being merely an empirical adjunct to the design process, to the level of a sociological source of design-enriching concepts.

However, it is not clear to us that this approach represents a suitable model for the creation of a hybrid management studies, at least at the present state of development. We have suggested that there is an alternative way of resolving the paradox of technomethodology that draws on a particular audience configuration. We have argued that in cases where the subjects of the research are identical to the audience, the researcher's involvement in transformational activity reinforces, rather than detracts, from the EM credentials of the research, by providing confirmation on the unique adequacy of the methods used.

For a management audience, such research makes a contribution towards what Donald Schön calls "reflective practice" (1983/1991). Here, as in the case of anthropological and technomethodological applications, the strong requirement of UA contributes to the viability of EM-informed management studies within the conjugal discipline. Thus, studies carried out under EM policies offer reflections on management practice that prioritise the concerns of managers over the theoretical predilections of researchers.

Schön suggests that a dilemma faces practitioners in every profession:

Shall the practitioner stay on the high, hard ground where he can practice rigorously, as he understands rigor, but where he is constrained to deal with problems of relatively little social importance? Or shall he descend to the swamp where he can engage the most important and challenging problems if he is willing to forsake technical rigor? (1983/1991: 42)

He proposes to resolve this dilemma by substituting the rigour of practical trial for professional criteria, where the latter are not viable. While recognising the power of this idea, we suggest that the studies considered in this paper represent an alternative solution; one in which the academic criterion applied insists upon the centrality of, rather than competing with, the knowledge-in-use of practitioners.

Like Schön's own, this approach focuses on the skills of professional practice, rather than the explicit bodies of knowledge espoused by professionals. However, in our view, even the

action research advocated by Schön carries theoretical baggage that can come between researchers and their understanding of managers' methods. Thus, for instance, Chris Argyris and Schön (1996) advocate a distinction between single and double loop learning, without explicating the relationship between this theoretical concept and researchers' practice. Whatever the didactic value of the concept for teaching managers to think more reflectively, it carries the implication that researchers assume (and therefore seek out) a form of unreflective learning among managers that, as educators, they can redress. By contrast, an ethnomethodology of management prompts researchers to focus on the problems that managers themselves find important. In doing so, we believe that EM has a significant contribution to make to action research, as the basis for a principled and productive relationship between academics and managers.

We have argued that much of the confusion over the policy of EM indifference stems from a failure to make an explicit recognition of the different audiences that exist for a particular piece of research and of the different criteria they employ in its evaluation. It is inevitable that these different audiences will sometimes require different glosses on the same piece of research. These different reporting practices should not detract from the EM credentials of the research. On the contrary, EM researchers must engage in many practices other than EM analysis, if they are to see anything at all (or indeed, to live in the world!). Thus, researchers may, for instance, sometimes be assistants to designers, sometimes addressing traditional sociological concerns, sometimes students of management, or sometimes engaged in the creation of hybrid disciplines. There should be no danger in engaging in these diverse activities, if analytic distinctions are maintained between them. Making and maintaining such distinctions is, we submit, a primary task for the EM community of researchers. In doing so, the EM audience for hybrid studies of work not only polices the rigorous application of EM policies and methods, but provides a community of scholars within which a comprehensive alternative sociology may begin to emerge.

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<sup>2</sup> We are indebted to an anonymous reviewer for bringing this important point to our attention. It is developed in the section that follows.